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THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

ECONOMICS AS A SCIENCE.[1]

THAT economic and social studies should be carried on with the scientific method and spirit is not likely to be denied by any one here present. And yet there are persons who would have us believe that these important fields of investigation are by nature incapable of such a treatment. Even among economic students we find many writers of the type of Cliffe Leslie, who definitely disclaim that economics is a science, and class it rather as a branch of history. There has always been more or less hesitation as to the place economic studies should occupy as between the historical and scientific poles. The tendency to lean toward the historical side is indicated by the fact that the American Economic Association has regularly met in conjunction with the American Historical Association, while the tendency toward the scientific side is indicated by the fact that this section exists as a branch of the American Association for the Advancement of Science.

Those who maintain that economics is not and never can be a true science base their contention on the fact that social phenomena are not constant, 'like,' they say, 'the phenomena of astronomy or physics,' but differ widely at different times and under different circumstances. They point out that the determination of prices

[1] Address of the chairman of Section I of the American Association for the Advancement of Science, at the Ithaca meeting, July, 1906.

under modern free competition is quite different from their determination under the medieval system of custom and status; that the remuneration of labor depends on what are the historical and legal institutions with respect to slavery, labor legislation, etc.; that the economic phenomena of to-day are not comparable with those of the times of the Greeks and Romans, nor are the phenomena in America comparable with those in Russia.

To one who is familiar with the spirit of science, however, these variations, so far from being objections, are really confirmations of the theory that economics is a science. For in all science it is fundamentally true that phenomena will 'differ according to circumstances,' and the office of the scientist is simply and solely to find out under what circumstances one set of phenomena will occur, and under what circumstances another set will occur. We could hardly claim that hydrostatics is not a science for the reason that in a mountain lake water is found to be stationary and at a level, whereas at Niagara it is found to be in motion and passing from one level to another; that whereas the water in a mill-race passes in a downward direction, the water which we draw in our houses moves through the pipes upward; that whereas, by means of a syphon, water may be induced to flow out of a receptacle, it will, in an ordinary tub, remain inert. The whole science of hydrostatics has developed as a consequence of the persistent effort to unravel these puzzles, and to-day we know not only that under different circumstances water will act in different ways, but we can formulate what are the precise conditions under which it will act in each separate manner.

In economic study we should in like manner apply ourselves to discover what conditions make the difference in the phenomena between modern and ancient or eastern and western civilizations, rather than content ourselves with the truism that they do differ. Much of the field has in fact already been covered. It is known, for instance, that under conditions of free contract and competition, the price of an article will be determined by the intersection of its supply and demand curves, and that, on the other hand, if the régime be one of monopoly, the price will be determined on the principle of 'what the traffic will bear,' in the manner so admirably shown by Cournot. In these cases the results are not absolute and unconditional, but depend on specified hypotheses. In this respect they are exactly similar to any other scientific result. The formula of science, as Newcomb points out, is always conditional—if A is true, then B is true. The formula of history, on the other hand, is unconditional—simply B is true.

If economics is a science, its truths must be conditional. Thus, the incidence of a tax on ground rent will lower the value of land, *provided* there are no counteracting causes. This does not assert that actually, after such a tax, the value of the land will fall, for in the meantime some opposing cause may have intervened, such as the discovery of an oil-well. Again, an increase of the quantity of circulating medium will raise prices proportionately, provided the velocity of circulation and the volume of business transactions remain the same. This 'quantity theory' does not assert that prices *do* rise after every increase in the quantity of money, and those who thus interpret it are guilty of the confusion already noted between conditional and unconditional truth—in other words, between a scientific law and a historical fact.

The confusion between historical and scientific truth is very common among economists, especially in the German historical school. While Roscher and his followers verbally classify economics as a sci-

ence, they leave no place for economic *theory*. Curiously enough, they imagine that by confining themselves to historical generalizations they are following the inductive method of Bacon. A closer study of Bacon's work would have revealed to them that true science does not consist in the mere grouping of historical phenomena. In fact, Bacon makes a distinction between what he calls the popular and the inductive method, or what may be preferably called, following the example of John Rae, the *systematic* and the *scientific*. The two are commonly confused, but are entirely distinct. 'System' consists in classifying phenomena; 'science' consists in discovering the laws to which they conform. System explains phenomena by means of what is common and familiar; science explains them by what is simple, however recondite. System is exemplified in such descriptive studies as grammar, descriptive geography and history; science is exemplified by such analytical studies as mathematics, physics, and latterly, biology. The classifier or system-maker is content with generalization of facts. These express the *usual* order of events, for instance, that the sun rises once a day. They do not express the reason or principle. The difference between system and science is the difference between a general fact and a necessary truth, between *rules* and *laws*. Exceptions to rules do not destroy them as rules; in fact, we say 'the exception *proves* the rule.' This, however, is not true of laws. A 'law' which has an exception is no law at all.

Many studies which are now scientific had their origin in what was originally systematic. The predecessors of the modern physicists classified bodies into 'light' and 'heavy.' Iron, they maintained, is heavy and therefore falls; fire is not heavy, and therefore rises. How different is this obsolete method of treating the subject from our modern analytic notion of gravity, or rate of increase of velocity, by means of which are explained both the falling of iron and the rising of 'fire.'

Similarly, the prototype of biology was 'natural history,' and consisted chiefly in the mere classification of animals and plants into species, genera, etc. Modern biology has supplanted such elaborate classification by introducing, through Darwin, the analytical ideas of heredity, variation and selection, and in this way the descriptive study of natural history has been converted into the true science of biology.

The same evolution which has been outlined in physical and biological science is doubtless taking place in economic science. Yet it must be confessed that few have yet mastered the distinction between a general fact and a scientific law. When we hear it stated as fundamental in political economy that skilled labor is better paid than unskilled, it is clear that this is merely a general rule and not a necessary law. The single fact that certain seamstresses, though skilled, are illpaid, is sufficient to disprove the statement as a necessary law, though it does not affect it as a general fact.

The historical school justly complains of the superficial character of the theories which have been sometimes offered. This objection holds, however, not against theory as such, but against *false* theories; and herein lies the virtue of Bacon's method. The inductive method, by which any theory of phenomena must be checked by reference to actual historical fact, thus forms the means of distinguishing between truth and falsity. Rejecting false theories is quite different from rejecting all theories. What is needed now in political economy is to rid ourselves of the false and superficial theories, on the one hand, which have been constructed *a priori* and irrespective of facts; and, on the other

hand, to release ourselves from the cheap empiricism of the historical school, who interpret their task as merely one of generalizing phenomena without analyzing them. In the words of the great philosopher, Hume, 'Our speculations can scarce ever be too fine, provided they be just.'

Science is one. The logic for economic science should be the logic for all science—a combination of induction and deduction. Facts are at once the test and the material of science, but laws are its ultimate goal. Laws are not facts, but the relations between facts. Newton's first law of motion, that a body tends to move uniformly in a straight line, is not a fact, nor is it a general expression of facts. Probably no particle in the universe has ever moved exactly in a straight line or with uniform velocity for so much as a single second. Yet it would be an error to conclude that Newton's law is unreal and untrue in actual nature. The law has an 'if' in it—'*if* a body were acted on by no force, or by perfectly balanced forces, its motion *would* be uniform in both rate and direction.' Withdrawn thus from actual events, Newton's law seems to the non-scientific mind to lose all objective truth. This again is an error. Newton's law is absolutely true to nature. The fact that it is conditional does not make it arbitrary. We are not free to replace it by the medieval opinion, viz., 'If a body is let alone it will gradually spend its force and slacken its speed.' This formulation, unlike Newton's law, would not stand the test of facts. A valid law is true at all times and places, in the sense that should the particular conditions arise, the prescribed result would follow, but not in the sense that the particular conditions must needs ever arise.

When the relations between scientific and historical truth are more fully realized, we may expect economic studies to make more rapid strides than have hitherto been possible. We shall recognize the two-fold nature of most practical economic problems, such as the present problems of trades-unions, insurance, railroad rate regulation and the tariff. These problems require first of all the study of historical facts, and secondly, the discovery of the relations to which these facts conform. When these two studies are complete we are prepared to take a third and final step, *prediction*. It is sometimes said that the ability to predict is the final test of science. But it is not a test of science only. Successful prediction requires two conditions: one is a knowledge of science—of what will happen under given circumstances; and the other, equally essential, is a knowledge of history—of the particular circumstances of the present moment, out of which the future, to be predicted, will grow. Failures of prediction are due to the lack of either of these two essential conditions.

An example of a failure of prediction due to imperfect knowledge of facts is found in the case of the closure of the Indian mints to silver in 1893. It was expected that the value of the silver rupee would be maintained at 16 pence. But no account was taken of the large coined hoards of silver among the natives. After these had been put into circulation the price of the rupee did eventually rise to 16 pence and has remained there ever since. In this case the failure of prediction at first was due, not to any defect in monetary *science*, but to ignorance of Indian *history*.

Usually, however, failures in economic prediction are due to the lack of scientific rather than of historical knowledge. In the civil war, when there was a premium on gold, the scientific explanation of which was really simple, the public attributed the premium to the machinations of speculators. Accordingly, Congress was induced to close the Gold Exchange, whereupon,

to the consternation of the framers of this foolish prohibition, the premium on gold soared higher than ever. The result was a hasty and shamefaced repeal.

Experience of this kind is too common in economic legislation. It serves as a warning that we should know something of economic science before venturing to tamper with economic conditions. The men who need this warning most of all are those who despise all 'theories' and call themselves 'practical.' It is they who legislate a measure one day and have to repeal it the next. A *truly* practical man can predict how a measure will work, and his power so to do requires not only what is called 'practical' but also what is called 'theoretical' knowledge; a knowledge, in short, not only of *history* but of *science*.

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SECTION D—MECHANICAL SCIENCE AND ENGINEERING.

THE meetings of the section were held in the lecture room of Sibley College of Mechanical Engineering and Mechanic Arts, of Cornell University, on Friday and Saturday, June 29 and 30. They were followed by the fourteenth annual meeting of the Society for the Promotion of Engineering Education, an affiliated society of the association.

In the absence of the vice-president, the retiring vice-president, Fred W. McNair, president of the Michigan College of Mines, acted as chairman of the section. Twenty-three members of the association registered as belonging to the section, while members of other sections attended some of the meetings. Experience gained at this meeting has shown that it is not conducive to the greatest attendance to have Sunday intervene between the meetings of the section and of the affiliated society.

The first two papers were by Byron B. Brackett, professor of physics and electrical engineering of the Thomas S. Clarkson Memorial School of Technology, Potsdam, N. Y. The first paper describes a 'Lamp Bank composed of Small Separate Units.' Each unit consists of twelve incandescent lamps arranged in a partially open box, 28 inches long and 10 inches square. The boxes are open enough to give perfect ventilation. On one side are placed switches for cutting in or out the lamps of that particular box, and for changing the grouping of the lamps into parallel, series or combination arrangements. As many of these units as are desired may be placed one upon the other in tiers, and as many tiers as are needed may be placed side by side to form a lamp bank of any capacity. Each unit is constructed to permit convenient interconnection with the adjacent ones. Thus, the large bank may be separated into smaller ones whenever desired and reassembled with the least possible loss of time and effort for special tests on large machines.

His second paper was on an 'Alternating Current Wave-form Apparatus.'

To set the armature accurately for the separate readings in the ballistic method of tracing the E.M.F. wave of an alternator, a double or duplicate brake apparatus is unusually satisfactory. Two similar clamp brakes with long arms are placed on the pulley side by side. With the arms held rigidly the desired readings for one observation are made. Then brake No. 1 is unclamped at the pulley, the end of its arm released and rotated up a short distance to a fixed stop. Brake No. 1 is now clamped to the pulley, brake No. 2 is unclamped, the end of No. 1 turned down to its original position and No. 2 is clamped again. Thus the armature has been rotated through a small angle, that may be computed with great accuracy from the length of the arms and the distance

through which the end of the arm of No. 1 is moved. The same method may be used wherever small accurate rotations of any shaft are desired and the same apparatus may be used on dynamos, engines and other forms of machinery.

A report was made by Mansfield Merriman, professor of civil engineering, Lehigh University, South Bethlehem, Pa., on 'Constant and Probable Errors in the Estimation of Linear Distances and Vertical Angles, as ascertained by 1,712 Observations on 128 Students.'

The observations were made by asking students to record their estimates of the length, width and thickness of a board, and also of the magnitude of several vertical angles. The angles were estimated in degrees and also by the ratio of horizontal to vertical projection. The following conclusions were drawn:

1. For lengths, about sixty per cent. of the estimates were too large, and the average constant error was always positive.

2. For angles, about eighty per cent. of the degree estimates, and about sixty per cent. of the ratio estimates, were too large.

3. The estimates of vertical angles by the ratio method are more accurate than those by degrees.

4. The probable error of a single estimate is larger than the average constant error.

5. The estimates of the freshmen class had much larger errors than those of other classes.

This interesting example of the application of laboratory methods to the classroom created much interest on the part of the engineering teachers present.

The next paper was read by the secretary, and described and discussed the results of 'Some Experiments on the Frictionless Orifice' and was by Horace Judd and Roy S. King, assistant professors of experimental engineering at Ohio State University. The paper is in sequence with one presented by one[1] of the authors at the St. Louis meeting, on 'Pitot Tubes; with Experimental Determinations of the Form and Velocity of the Jets,' and which was published in the *Engineering News* of March 31, 1904, page 318.

The experiments described in the paper were made on five frictionless orifices, ranging in size from three fourths inch to two and one half inches in diameter. The term 'frictionless orifice' is one which has been commonly used in the mechanical laboratory at the Ohio State University for many years. It is the same as what some of the text-books call 'an orifice in a thin plate,' and indicates that there is no appreciable loss by friction by a stream of water flowing through such an orifice. So far as known, the term was first employed as early as 1873 by Professor S. W. Robinson, emeritus professor of mechanical engineering at the Ohio State University, and will be found on page 552 of Vol. VI. of the Geological Reports of Ohio, 1886, in his report on the 'Measurement of Gas Wells and other Gas Streams and the Piping of Natural Gas.'

The facilities available consisted of a closed standpipe into which several pumps could force water. A horizontal drum was connected near the bottom of the standpipe, and in the end of the drum the orifice to be tested was secured; the coordinates of the issuing jets were measured by the hydraulic micrometer caliper, described in a paper presented by Professor Wm. T. Magruder at the St. Louis meeting of the association.[2] The water was caught and measured in a calibrated cistern.

The experiments were made to find:

[1] Science, N. S., Vol. XIX., No. 479, March 4, 1904, p. 364.

[2] Science, Vol. XIX., No. 479, March 4, 1904, p. 364.

1. The coefficient of discharge by direct measurement in a calibrated hydraulic cistern, (*a*) to verify existing constants, and (*b*) to find the coefficient of velocity for comparison with that given by the Pitot tube, and (*c*) to verify its constant. The results show a decrease in the coefficient of discharge of 2.5 per cent. with an increase in the diameter of the orifice from three fourths inch to two and one half inches. The average value obtained was 0.60664, and is about 1.8 per cent. greater than that given in Merriman's 'Hydraulics' (0.5960). Compared with that of Bovey (0.6000), the result obtained with the jet from the one inch orifice (0.6097) is 1.6 per cent. higher.

2. The shape of the jet in the vicinity of the least section as found by exploring the contour of the jet by the special micrometer caliper. It was found that the distance from the face of the orifice to the least section of the jet varied from 1.6 radii for the three fourths inch orifice to about one radius for that two and one half inches in diameter. Text-books on hydraulics give this distance as being one radius, while Bazin found no 'minimum section' of the jet flowing through a large orifice and under low head.

3. The diameter at least section, as found by the special caliper for four different positions, 45° apart. From the average diameters, the coefficients of contraction were computed, and they were found to decrease from 0.6134 for the three fourths inch orifice to 0.5955 for the two and one half inch orifice, with an average value of 0.60674. As calculated from the average results for the coefficients of discharge, the coefficient of velocity is the quotient of the coefficients of discharge and contraction, or 0.60664 divided by 0.60674 equals 0.99983. From this result it is thought that the term 'frictionless orifice' is justifiable.

4. The effect of increase of static pressure on least section. No appreciable increase was found in the diameter of the least section of the jet from a one inch orifice by increasing the pressure from 5 pounds to 100 pounds; and, for pressures as high as 40 pounds, only a slight increase was noticed for the orifices one and one half inches to two and one half inches in diameter. This latter was probably due in part to the increased roughness of the contour of the larger streams.

5. The velocity in the least section was found by traversing the jet with a one fourths inch Pitot tube used in connection with a differential manometer. It is thought that the coefficient of velocity as determined by the Pitot tube is unity within a negligible fraction of 1 per cent. and that the velocity is uniform throughout the section. As calculated from the average values of the coefficients of discharge and contraction as determined by experiment, the coefficient of velocity was found to be 0.99983. This compares very well with the average value of the coefficient of velocity, 0.99993, determined by means of the Pitot tube. This would seem to verify the statement that the Pitot constant is unity.

The paper shows that it is the result of most careful and elaborate investigations. It also shows the inaccuracy of certain statements which are still being published in books on hydraulics concerning the vena contracta. While adding new facts to our knowledge, it gives us a method for measuring quantities of water discharged, and with a probable error which is known to be small and less than the probable error of observation. The paper is worthy of careful study and attention. It will probably be published in the *Engineering News*.

The next paper on the program was on 'Some Aspects of the Panama Canal,' by Wm. H. Burr, professor of civil engineering, Columbia University, New York, N.

Y., and a former member of the Isthmian Canal Commission. As the paper has been published in full, and will be found on page 71 of the issue of SCIENCE for July 20, 1906, its contents need not be detailed here. Suffice it to say that the recent dreadful earthquake disaster at San Francisco constitutes the gravest warning in human experience of the advisability of constructing this canal in such a way as to give it the greatest degree of immunity from the results of any convulsions of nature, and the still more recent earthquake shocks on the isthmus should be sufficient to set even politicians to thinking what would be at least one result of an earthquake to the gates and walls of a lock canal having 85 feet of lift.

In this connection it should be noted that while no efforts have been made to secure authors of papers who were in favor of the sea-level canal, yet all the papers which have been presented before this section in the last few years have logically given most excellent reasons why the canal should be built with the fewest possible locks and of the least lift. Time will decide whether the prediction is correct which has been made before this section that the high lock level canal will have to be rebuilt before it has been in use ten years.

Henry T. Eddy, professor of engineering and mechanics, University of Minnesota, Minneapolis, Minn., presented a paper on 'The Theory of the Flexure of the Symmetrically Loaded Plate, and the Heavy Horizontal Disc of Varying Thickness.' Although the theory of the elastic flexure of the thin flat plate of uniform thickness has been long known to the same degree of approximation as that of the common theory of flexure of beams, the theory of the heavy horizontal disc on a vertical axis and of a thickness decreasing from the axis of the rim has, up to the present time, been in a somewhat unsatisfactory state from the point of view of the engineer who would design discs like those employed in the Curtis steam turbine. The object of this paper is to make a contribution to this theory, which it is hoped will facilitate both the algebraic and numerical treatment of such units of construction.

A paper was read and illustrated by Henry S. Jacoby, professor of bridge engineering, Cornell University, Ithaca, N. Y., on the 'Applicability of Displacement Diagrams in the Design of the Flanges of Arch Ribs with Solid Webs.'

The usual method of finding the stresses in the flanges of arch ribs with solid web plates involves the computation of bending moments and axial thrusts at a number of sections due to dead and live loads, and also when there are less than three hinges, for changes of temperature and rib shortening. The formulas for the determination of the horizontal reactions, while comparatively simple for ribs with a parabolic axis, are complex for a circular axis, and entirely impracticable for other curves such as three or five centered ovals. The method proposed is to assume the solid web plate to be temporarily replaced by an imaginary inelastic system of open webbing, the panel points of each chord being located in the centers of gravity of the corresponding flange sections. It allows the horizontal reactions to be found by means of displacement diagrams which apply to any form of arch rib. The method also affords the simplest means of finding the deflections under any loading.

'Some Notes on the Teaching of the Cyclic Analysis of the Gas Engine' was next presented by Arthur J. Wood, assistant professor of experimental engineering, Pennsylvania State College, State College, Pa.

As a basis for this study and discussion, the results of tests of a five-horse-power

gasoline engine were presented in full. The distribution of heat thus obtained is compared with results obtained by an entropy-temperature analysis of one of the indicator diagrams in which the losses are shown graphically. The paper brings out the method for finding the temperature at any point in the card; the heat of the mixture in the cylinder; the average value for the exponent of the adiabatic curves of expansion and of compression and values for different parts of these curves; the method of obtaining the entropy for all points; and a comparison of results of some recent tests at the Pennsylvania State College.

Certain conclusions are drawn from the study of the subject: (1) the losses can not be computed accurately unless the data are taken more completely and with greater care than is usual with such tests; (2) about 20 per cent. of the heat of the gasoline vapor is available for useful work at full load; (3) the common method of obtaining, by test, the heat in the exhaust gases is not satisfactory; (4) the teaching of the entropy-temperature analysis without requiring the student to make the analysis is a pure waste of time (the average student in the senior class ought to master the whole theory in a few hours, and make the analysis in a few days' time, and this will open to him some of the neatest applications of thermodynamics; a complete analysis by a student is presented in the paper); (5) the value of the exponent of the true adiabatic curve is obtained from the ratio of the specific heat at constant pressure to the specific heat at constant volume, and if this curve is constructed it will bring out at a glance where heat is taken in or given out, without resorting to the entropy-temperature diagram. Brief reference is made to the method known as 'cyclic analysis,' in which the principal results as mean effective pressure are obtained from the heat put in, the initial temperature, the value of the specific heat at constant volume and the temperature after compression. The author has found this method to contain nothing attractive as a system for teaching, because, (1) it makes the student a mere machine, substituting in derived formulas; (2) the value of the heat for each cycle can not be obtained accurately by this method; and (3) it does not open up clear, well-defined lines of application of the principles of thermodynamics. The paper as a whole aims to make it clear what data must be obtained in order to make an entropy temperature analysis and how it may be made practical as a method of instruction and valuable in design. It further shows how the principles of thermodynamics can be made as clear in showing changes of heat energy as of mechanical energy. In all this the writer does not depreciate the value of the purely analytical methods.

The next paper was by W. W. Churchill, vice-president and chief engineer of the Westinghouse, Church, Kerr Company of New York, on 'The Preservation of Surface Condenser Tubes in Plants using Salt or Contaminated Water Circulation.' The paper will be published in full in the October issue of *Power*.

Owing to electrolytic and galvanic action on the tubes of a surface condenser using water containing salt or sewage for circulation, the use of surface condensers in large power plants has been very largely abandoned as useless. Under ordinary circumstances the water bill of such a plant as that described for the Long Island City power house of the Pennsylvania Railroad, containing when fully constructed fourteen 5,500 K.W. generating sets, would be in the neighborhood of $100,000. An investigation was undertaken of possible methods for preventing the rapid destruction of the condenser tubes and parts of the condenser.

The most competent opinion that could be obtained was that if the differences of potential due to stray ground traction or other currents could be adjusted to be within three volts or less, the difficulty would cease, as the trouble was all due to stray grounded currents from electric railways. To that end, the stray currents were measured, and at times a difference of potential of nine volts higher than the harbor was discovered between the railroad rails in the freight yard and the river, producing a considerable flow of current from all parts of the water piping and other metallic substances in the vicinity of the power house site to the harbor.

In order to properly study the real conditions, a number of glass jars were provided and various combinations of metals were immersed in samples of water taken from the river, of sea water, and of pure water. The action of the river water was much more violent than ordinary sea water. It was further observed that there was a local action going on which was galvanic, and that the amount of stray currents caused the galvanic action to be exceedingly violent, and that thin copper tubes were destroyed at a very rapid rate. They would be punctured in from four to five days, which would make their use fatal in commercial practise. It is thus evident that there was a violent action between the zinc and copper of the brass tubes which were in contact with the electrolyte. Even when unconnected, or electrically separated, plates of brass were placed in this electrolyte, any projections were promptly destroyed. But if an electric battery having a pressure exceeding that of the couple in the East River water was caused to act to produce a counter current, and having a pressure exceeding that of the galvanic couple, the capacity of this electrolyte to drive off atoms of the mechanically combined metals of the alloys used was overcome and corrosion was arrested. To do this it was planned in this case, where this couple is about 0.4 of a volt, to put a counter pressure of 0.6 of a volt on the anode. In order to insulate the condenser as much as possible from stray currents from the railway, the joints in the piping and the ground contacts were insulated, and even the large water connections were lined with glass melted on to the surface. To furnish the desired electromotive force a three-K.W. motor-generator was installed and provided with switchboard apparatus and appliances, together with ammeters and voltmeters, and a connection to the outlying anode in the condensing supply intake at its harbor end. This led to considerable discussion as to the methods and reasons why the corrosion was prevented. Reference was made to Faraday's laws, and to the results of the experiments of Helmholtz and Clausius. The best explanation is given in Professor Oliver J. Lodge's 'Modern Views of Electricity.' All the apparatus was designed in accordance with his theory and statements. Its operation has extended over a period of fourteen months, and with the exception of about ten tubes which have become pitted, the results have been satisfactory. When the condenser was planned, the condenser manufacturer was told to slope the tubes down one inch in the direction of flow; but when they performed the act, they forgot that the middle inclination, if parallel to the first and third passes, would then be uphill for the circulating water, and that when the condenser was shut down all the water would drain out of this middle section except that in the bottom tubes. These bottom tubes, it has been found, have become somewhat corroded, although not seriously.

The cause for this result having now been found, its prevention has been effected by perforating the diaphragm in the

condenser head to permit prompt drainage of these tubes when the condenser is taken out of active operation. Considerable difficulty has been encountered in getting the system operated properly at the voltage suitable for the prevention of galvanic action in the hands of the regular operators, as they persisted for a long period in reading amperes instead of volts in adjusting the currents, thus not providing except by accident for the requirements. The efficiency of the apparatus amply justifies the expense of its installation, while its operation is not expensive and the plant here described will now be followed by other protecting plants of the same character.

This paper forms a most excellent illustration of the application of theoretical physics to the solution of a concrete problem in commercial engineering. The saving of $100,000 a year in water rates at the expenditure of $250, or even more, per year, in a scientific preventive should be a sufficient answer to the commercial man who asks 'Does science pay?'

The paper on "The Justification of the Use of the Expression 'Engineering Mathematics,'" by Arthur E. Haynes, professor of engineering mathematics, University of Minnesota, Minneapolis, Minn., describes the conditions as they exist in a large university of the central west wherein mathematics is taught to students pursuing all kinds and classes of courses. The paper will be published in full in the *Proceedings of the Society for the Promotion of Engineering Education.*

The last paper, and in some respects one of the most interesting and valuable papers presented before Section D, was by J. J. Clark, manager of the text-book department of the International Correspondence Schools, Scranton, Pa., on 'The Correspondence School: Its Relation to Technical Education, and Some of Its Results.' The paper will be published in full in the *Proceedings of the Society for the Promotion of Engineering Education* and will doubtless be abstracted by the technical press.

When Professor Edgar Marburg read a paper[3] in 1899 on 'The Correspondence School in Technical Education' there were about 80,000 students enrolled in the International Correspondence Schools. In the following year this number had increased to 181,000. Owing to a number of circumstances, it was impossible at that time to furnish reliable figures in regard to the work being accomplished. Hence the reports which were made were both unsatisfactory and unjust to the correspondence school. Since December 31, 1899, the school has grown at a rate of more than 100,000 per annum, the total number enrolled on June 27, 1906, was 902,906. After relating something of the history of the growth of the schools and their methods of securing students, the author describes their system of education as being based on ideas that are almost the direct opposite to those practised by the regular schools and colleges. The regular technical school or college aims to educate a man broadly; the aim of the correspondence schools of the country is to educate him along some one particular line, and to make each course fit the particular needs of the student who takes it. Hence, with a few exceptions, almost every one of the courses is a special course.

The author then describes other methods of conducting correspondence schools by the use of the regular text-books and correspondence concerning the difficulties encountered by the student. The author states that such schools have always failed or made very little progress, 'for the reason that the ordinary text-book is not

[3] *Proceedings of the Society for the Promotion of Engineering Education*, Vol. VII., p. 80.

adapted to the use of the person studying by himself.' Another method is that of the University of Chicago, where students may take part of their regular college course at home. He can not, however, get his degree without taking the balance in residence. This plan offers few advantages over the regular college course. The only requirement necessary to become a student of the International Corespondence School is the ability to read the English language and to write it sufficiently well to be understood, or to possess the services of some one capable and willing to translate from the text-books and write out the dictation in English of the foreign-speaking student who is unable to use the English language.

The text-books differ, even on the same subject for the courses for which they are to be used; not only on the method of treatment, but principally in the examples given and the illustrations cited.

The author gives a number of reasons why students do not complete their courses, and also mentions cases of students who admit deriving much benefit from the course but who have never submitted an examination paper for correction. Their instruction has been obtained solely from the bound volumes which were furnished them when they enrolled. About 60 per cent. of the students send in one or more pieces of work. About two thirds of the students pay in full for their courses. While the present enrollment is a little over 900,000, the actual number of students is probably 60 per cent of this, or 540,000. During the year ending May 31, 1906, the total number of pieces of work received from students was 716,952. About one sixth of the number of active students have completed about one third or more of their course as shown by the records. The number of students who have entirely completed their courses, passed their final examinations and been awarded a certificate or diploma was 12,143 on June 27, or about 2.6 per cent. of the total number of active students. Between February 7 and April 21 the number of diplomas issued averaged 240 per month. The average for next year is estimated as high as 300 per month, or 2,700 diplomas for the year from this one correspondence school. As this number is about the same as the number of engineering degrees conferred per annum by all the technical colleges of the country, the amount of work that this represents, and the amount of education which is being obtained can be somewhat appreciated.

The social sides of the meeting were very well cared for by the committees of Cornell University, and have been described in the report of the general secretary. A special excursion was tendered to the section by the committee on Saturday afternoon in the form of a steamboat ride on the steamer *Iroquois*, down the lake, and which included a visit to the Remington Salt Works near Ithaca. Here steam is generated and used in steam engines for the generation of electric power, and the exhaust steam is used in the vacuum pans of the salt works. A feature of the meeting which appealed to every one was the delightful entertainment accorded to the members in the houses of the fraternities. Altogether the meeting was most enjoyable and profitable, and both the section and the Society for the Promotion of Engineering Education desire to extend their special thanks to the trustees, the faculty and to the fraternities who opened their houses at Cornell University.

Wm. T. Magruder,
Secretary.

Ohio State University.

SECTION G—BOTANY.

In accordance with arrangements previously made the section met on June 29 for the transaction of business and the dis-

cussion of the future relations of the section to the Botanical Society of America. Professor George F. Atkinson was elected secretary *pro tempore* in the absence of Professor F. E. Lloyd.

The principal feature of the meetings on the following days were the splendid excursions which had been arranged by the local committee. Adequate transportation, well-appointed lunches, and the presence of the botanists of the botanical department of Cornell University, who were familiar with the flora, made these outings a very profitable feature, and called out an attendance as large as that customarily seen at technical sessions.

On Saturday, June 30, a party of twenty-three visited the atoll moor near Chicago, eighteen miles from Ithaca. For a distance of twelve or fifteen miles on the route the glacial 'dumps' or terminal moraines were visible to the party, showing in many cases the 'kettles' or 'pot holes' or 'cat holes,' as they are variously termed, many of which support characteristic moor vegetation. The atoll moor which was first visited was the site of a former large glacial pond which is now nearly filled by the growth of vegetation, there being a deep central small pond and an outer moat containing water at the shore of the original pond.

After lunch on the west bank of the moor Professor Atkinson gave a brief talk describing the topography of the moor and outlining the different theories which have been suggested in explanation of the peculiar topography which has been formed by the vegetation filling in. Dr. Wiegand called attention to the principal species of plants and plant formations, pointing out the work of each in moor formation.

The party then explored the vegetation of the border along one side, which is made up of grasses, sedges and herbs. Here a large number of Uredineæ were found among which were five or six heterœcious species with the æcidial stage and the uredo stage growing on adjacent hosts, as pointed out by Dr. Arthur. The *Cassandra* formation, which covers the larger part of the moor inside of the moat, was then explored. In reaching this it was necessary to cross the water in the outer moat and also a narrow elevated ridge just inside of this, which for a great distance is covered by *Spiræa salicæfolia.* Among the *Cassandra* were quantities of *Sphagnum,* and many other plants lesser in number, as *Andromeda polifolia, Vaccinium macrocarpon* (cranberry), etc.

From this moor the party was led to one of the smaller and deep kettles, which contains at this season but little water. The zonal formation of the vegetation is also characteristic here: the center is a sedge and grass formation, the border an *Isoetes* formation, which at this time was partly submerged and partly on dry land. Between this and the central grass formation were partially submerged aquatic plants.

The party next drove to Malloryville moor. On one of the banks of this moor is a bed of *Pteris aquilina* (brake) which every year presents numerous cases of apospory, though the season was a little early for good examples. This moor is a high moor. The center is occupied with *Andromeda polifolia* and scattering *Cassandra,* with a deep bed of sphagnum. Pitcher plants, orchids, etc., were observed. Upon one side (south) the *Andromeda* is each year attacked by a parasitic fungus, *Exobasidium,* which causes a regular hypertrophy of the leaves. The affected shoots are colored a bright red, and the leaves instead of being thick, narrow and with inrolled edges, are thinner, broadly elliptical and flat, taking on exactly the shape of the *Cassandra* leaves, so that but for the

color it would suggest a 'bud shoot' of a *Cassandra* from an *Andromeda*. The appearance of two generic types in the branches of one shoot is very striking.

On Monday, July 2, a party of thirty-four drove to Enfield Gorge, alighting at the foot of the gorge, while the carriages were taken to the head of the gorge, a little more than two miles distant. The numerous successive falls and cascades were observed, also the geological formations, but especially the vegetation. There were numerous liverworts in the wet rock walls. In one place *Preissia* and *Conocephalus* are abundant and are usually brought into zonal formation by the different moisture conditions which vary according to the surface contour of the perpendicular rock wall. At a short distance *Pellia* usually grows where there is a greater quantity of water dripping down the rock. This year there is more moisture than usual. The dripping water is so abundant that it is killing out the *Preissia* and *Conocephalus* in certain spots and the *Pellia* is coming in and overgrowing them. These features were very easily demonstrated. Higher up on the clay bank at this place *Blasia* is abundant, and on the flat rocks below *Marchantia* was found.

Near the upper end of the gorge was an abundance of the fern, *Pellæa gracilis*, on the moist rocks. The vegetation of the small alluvial plains here and there was rich in forest and shade plants which afforded an excellent opportunity for observation of 'mosaics' and various ecological features.

The interest shown in a meeting in which the out-of-door features occupied so prominent a place suggests the desirability of their annual repetition, although it is by no means to be taken for granted that field excursions might always be organized under such favorable circumstances as those offered by Ithaca and the Cornell botanists.

D. T. MacDougal,
Chairman Section G.

SCIENTIFIC BOOKS.

Der Bau des Fixsternsystems mit besonderer Beruecksichtigung der photometrischen Resultate. Von Dr. Hermann Kobold. Friedrich Vieweg und Sohn. 1906. M. 6.50.

This volume is No. 11 of a series, 'Die Wissenschaft,' whose declared purpose is to place before the public, from time to time, a digest of the progress that has been made in definite departments of scientific research. Designed alike for the instruction of the general reader and for the orientation of the professional student in his own field, Dr. Kobold's work must be regarded as upon the whole an eminently satisfactory achievement. It is, indeed, probable that competent critics will dissent vigorously from some of his conclusions and will regard as far from final his judgment upon much of the conflicting evidence marshaled in the text. But with all due reserve in these respects the book possesses great merit both as a compilation of data relative to the structure of the stellar system and as a summary of current theorizing upon that data.

In some twoscore pages there is passed in brief review the methods of determining such fundamental data as the position, brightness and color of individual stars, stellar spectra, parallaxes, proper motions and the apparent distribution of stars upon the sky. Then follows the backbone of the work, a hundred pages devoted to a critical consideration of the present state of knowledge along these several lines, with particular stress upon problems of stellar motion. The author here defends the thesis, supposed to be original with him, although recently brought into prominence by Kapteyn, that the stellar motions can no longer be regarded as lawless in their arrangement, directed in equal measure toward all parts of the celestial compass. Rather must we consider them as having a definite relation to the Milky Way, the exact nature

of that relation being a large problem for future study, while as an immediate consequence of the altered concept we have doubt cast upon the supposed determinations of the sun's motion in space. At this point, as in most discussions of the kind, we miss a frank recognition of the relativity of all motion, and its corollary that the direction and velocity of the motion to be imputed to the sun is purely a matter of convention and definition, depending upon the choice of an origin to which that motion shall be referred. The author appears to regard the solar motion as something fixed in the order of nature, which should be found always the same, barring accidental errors, from any considerable group of stars used for its determination.

To the non-professional reader, doubtless, the most interesting part of the work will be found in the third section, devoted to the construction of the heavens. Without ignoring the work of others, Easton, Kapteyn, Schiaparelli, Stratonoff, etc., Seeliger's discussion of the distribution of the Durchmusterung stars here plays the principal rôle and is described by the author as 'a complete solution of the problem'; which 'determines the universe as revealed to us in the enumeration of the stars,' etc. The character of the finite and definitely limited universe thus revealed is described as follows:

> Throughout a finite space of spherical form there are distributed bodies of widely varying mass and widely varying physical condition. Amidst gaseous nebulæ of extraordinarily low temperature are placed other bodies strongly condensed and glowing hot. The arrangement of these individual masses is not one of haphazard uniformity, but, as swarms, they are clustered about individual centers which, in loose relation one to another, are arranged in the shape of a great spiral with multiple arms. In the remoter parts of this spiral the hotter and gaseous stars predominate, while near the center of the spiral is the sun and its more closely related stars, which for the most part resemble it in physical condition. Many of these latter share in the motion of the sun along the fundamental plane of the spiral, *i. e.*, the milky way. There are also numerous other groups of stars possessing a common motion in the plane of the galaxy.

The doctrine of a demonstrably limited stellar system thus presented is avowedly based upon statistics of the distribution of stars brighter than the tenth magnitude, and as these stars constitute, both in number and extent, only a small fraction of the visible universe one is tempted to question the soundness of that logic which extends to the larger aggregate, conclusions of an empirical character derived from a minute fraction of the whole. Indeed, conservatism in this respect seems the more required, since, as is conceded by Kobold, the apparent distribution of the fainter stars is quite unlike that of those from which his conclusions are drawn. Even for these brighter stars it is probable that the supposed indication of a limited and finite system is fallacious and arises from the tacit assumption that the fainter stars appear faint only by reason of their greater distance from the earth. It has been recently shown that such is not the case, the stars of the fainter magnitudes being intrinsically less luminous than those of the brighter classes.

At page 215 of the text Dr. Kobold falls into serious error in comparing Kapteyn's empirical formula for the probable parallax of stars of determined proper motion and brightness with Comstock's determination 0.0045″, as the mean parallax of a considerable group of stars of the average magnitude 10.5. According to Kobold, *'Kapteyn's Formel nur 0.0016″ verlangt,'* for this group of stars, while in fact the formula furnishes for this case 0.0042″, in excellent agreement with the observed value. It is only justice to Dr. Kobold to state that no other similar case has come within the reviewer's note.

G. C. C.

SCIENTIFIC JOURNALS AND ARTICLES.

The American Naturalist for August contains articles on 'Volant Adaptations in Vertebrates,' by R. S. Lull; 'External Morphology of the Dugong,' by H. Dexler and L. Freund, and 'Reproduction of *Metridium marginatum* by Fragmental Fission,' by M. L. Hammatt. Dr. Lull notes that volant evolution has occurred seventeen times, seven of these instances being for true flight. Dr.

Lull will pardon us for saying that there seems no good reason to suppose that the enormous occipital crest of Pteranodon was needed to keep the animal head to wind; were this the case it would have inevitably tripped the creature up when it wore ship. There is a slip of the pen in the statement that in birds 'The scapulæ and coracoids fuse with each other firmly,' since actual fusion only takes place in birds that do not fly, and not in all of these. The specific name of the flying frog is *reinwardtii* not *reinharti*.

We will also put a note to the quotation from Professor Moseley on page 539 and say that the dropping of the wings by the albatross is very likely for steering or balancing and that often one wing only, or the tip of one wing is dropped.

The authors of the article on the dugong note the lack of good figures of this animal, but fail to entirely remedy this defect, as the accompanying figures are few and not very good.

The Museums Journal of Great Britain for July contains the address of the president of the Museums Association, Dr. W. E. Hoyle, given at the Bristol meeting. This was devoted to 'The Education of a Curator,' is both instructive and interesting, and should be widely read. First among the qualifications of a curator Dr. Hoyle places 'general culture, tact and courtesy; an ability to suffer fools gladly.' Evidently the experience of Dr. Hoyle has been similar to that of other curators and he has our sympathy. It is interesting to note the value accorded to an acquaintance with the practical side of museum work, such as planning cases, structure of locks, cataloguing, taxidermy and the preparation of skeletons, since Dr. Hoyle has been so successful as a museum administrator that he is qualified to 'talk by the book.' Reference is made to the small salaries of museum officers, and in this respect matters are probably better in the United States than in Great Britain.

Bird Lore for July-August has articles on 'A Kingbird Family,' 'My Experience With a Blue-headed Vireo,' 'A Bit of Robin History' and 'The Yellow-breasted Chat.' W. W. Cooke contributes the seventeenth paper on 'The Migration of Warblers' and in 'The Audubon Societies' is given a summary of the laws for the protection of birds, or for the abolition of proper protection, likely to come up at the next legislative session in various states.

The Educational Leaflet, No. 21, is devoted to the scarlet tanager.

The Zoological Society Bulletin for July is termed the Reptile Number, being mainly devoted to a consideration of the more important reptiles now on exhibition. Under 'Methods of Exhibiting Reptiles' it is stated that many classes visit the park and that everything possible is done to assist them in obtaining correct information, children being taught that most snakes are harmless and encouraged to handle certain species. African reptiles are unusually well represented in the society's collections. There is a good illustration of the young two-horned African rhinoceros recently received, this being the first brought to this country in the last eight years.

The American Museum Journal for July has for its more important articles a detailed account of 'The Willamette Meteorite,' by E. O. Hovey, a notice of the commencement of work on the new west wing, and the concluding part of Mr. Chapman's 'List of Birds found within Fifty Miles of the American Museum of Natural History, New York City.' This is illustrated and accompanied by a list of the principal papers relating to the birds of the vicinity of New York City. The two parts are published together as Guide Leaflet No. 22.

The collection of woods has been embellished by the addition of copies of the leaves and flowers of the magnolias, the exhibit illustrating the life of the Plains Indians has been installed and a very perfect example of the jaws and head armature of the giant extinct fish *Dinichthys* placed on exhibition. There are many interesting notes on other work of the museum.

DISCUSSION AND CORRESPONDENCE.

THE SMITHSONIAN INSTITUTION AND THE MUSEUM IDEA.

THE discussion opened by David Fairchild and continued by David D. Mottier,[1] urging the divorce of the museum idea from the Smithsonian Institution, appears to me a hopeful sign, indicating an awakening of the scientific public to the underlying deeper question: Has the Smithsonian Institution been managed in accordance with the will of Smithson or has this country proved faithless to the trust freely accepted?

For almost half a century I have taken note of the Smithsonian Institution. I knew Joseph Henry personally quite well and in many ways assisted and was helped. I know his 'History of the Beginnings of the Institution,' which is also quite well known abroad, having been republished extensively in Europe; for example, *Cosmos,* IIe Series, T. III., Paris, 1866, pp. 723–760. The 'museum idea' is fully considered therein and everything local or even national is shown to be in conflict with the intention of Smithson.

Under the successors of Joseph Henry, the institution has gradually ceased to conform to the founder's intentions. Congress has been called upon to furnish money—and our Billion Dollar Congress has responded most liberally. A national museum has been developed, a new four-million-dollar building is now going up for the same; a zoological garden and an astrophysical observatory have been established; finally, costly experiments on flying machines have been provided for by Congress, all under the management of the secretary of the Smithsonian Institution, who is not an officer of the nation, but elected as executive officer of the Smithson Trust and paid exclusively from the Smithson fund.

It is not necessary to consider the qualifications of the successors to Joseph Henry. But it can not be denied that they were first of all *specialists,* the one in fishes, the other in stars. Thus they were not naturally predisposed for the broad object of the institution: the increase and diffusion of knowledge among men. Furthermore, each of the new national institutions established requires the full time and energy of a first-class man; this is the experience of other countries, where such national scientific institutions have long preceded and much excel ours. It can, therefore, not be questioned: both parties, the nation and the Smithsonian, have lost heavily by the attempt to administrate and plan our national institutions at the cost of the Smithsonian Fund.

The costly national institutions have not given the nation such a return in results as the outlay entitles us to expect. The putting of all the work of many able men on the shoulders of the one secretary of the Smithsonian Institution has necessarily crippled the national institutions, while it has equally necessarily made it impossible for that officer to give thought or time to the foundation of Smithson from the funds of which he drew his salary and to which he, therefore, owes in duty both all his time and his entire energy. That both the Smithsonian and the national institutions have thus been crippled can not be denied; specially glaring instances have come to my personal knowledge and have been referred to in some of my publications.

In brief, the following are the main facts of the condition that actually confronts us:

1. Congress appropriates millions of dollars for certain national scientific institutions, located at Washington, but depends for their planning and administration on the time and ability of one single man who is not in any sense a national officer nor receiving salary from the national funds; this one man serves as well as one man can do the work really requiring a number of men, each one a specialist in the line represented by each one of these different national institutions.

2. Congress has accepted Smithson's trust and funds, and through a board of regents manages the institution of Smithson. This board elects a secretary as executive officer and pays him a salary out of the Smithson Fund. His work necessarily demands the full time and all the power of a most able man of the broadest possible education and the highest mental endowment; for Smithson demanded his institution to work 'for the in-

[1] SCIENCE, June 8 and July 27.

crease and diffusion of knowledge among men.'

The British government refused to accept this trust from the Englishman Smithson, deeming the condition too broad and too difficult to comply with. The Congress of the United States thereafter was made the same offer by Smithson on the same conditions and accepted this trust on these conditions from Smithson while he was alive; this trust we are in honor bound to meet in every particular, now that Smithson is dead.

Are we faithful to this trust when we pay the secretary of the Smithsonian Institution out of the Smithson Fund—and then compel him to administrate our own large national institutions and thereby force him to neglect the Smithsonian Institution proper? To what extent this has been done is likely soon to become known throughout the scientific world in connection with questions that have thus far been suppressed here at home, but will come out by the enforcement of a recent act of Congress.

It seems to me that it is not merely a question of 'divorcing the museum idea' from the Smithsonian Institution, but to put a stop to the robbing of the Smithson Fund and to the nullification of the Smithson will. We certainly should administer our own national museums, zoological gardens, astrophysical observatories and flying machines, all paid for from national funds, granted by Congress; we should select the best man for each one of these duties and pay him from our own United States funds for his work. To take the pay for this our work in any manner or form from the Smithson Fund is to rob the grave of Smithson. It is a national disgrace that should cease the instant it is realized to exist.

But we should not only cease to rob the grave of Smithson; we should also again make an effort to comply with his conditions and to realize, in his name, so far as it be possible, his ideal: to increase and diffuse knowledge among men throughout the world.

Let us at least try to do that as well and as faithfully as it was done under the administration of the first secretary of the Smithsonian Institution, Joseph Henry.

Gustavus D. Hinrichs.
4106 Shenandoah Avenue,
St. Louis, Mo.,
August 17, 1906.

Is not the Smithsonian an Institute of Research?

To the Editor of Science: In your issues of June 8 and July 27 I note communications from David Fairchild and David M. Mottier in regard to certain changes they would like to see made in the work of the Smithsonian Institution.

I wish to take exception to one or two statements made by these gentlemen, though I entirely agree with some of their suggestions.

In the first place, both articles *imply* that the Smithsonian Institution is not now a place where important research is being done. Surely if one will but glance over the last 'Annual Report of the Smithsonian Institution' and the last volume of the 'Proceedings of the National Museum,' some sixteen hundred pages altogether, he will be forced to the conclusion that research is being done at the Smithsonian Institution. Note also some of the important monographs that have appeared under the auspices of the Smithsonian, Dr. True's recent work on the whales, for example. As is pointed out by the assistant secretary, the time of the regular staff is largely taken up with administrative duties, but, in spite of this fact, a very considerable amount of research is accomplished by these men. Besides the investigations carried on by the regular staff of the institution a large amount of research is done under the auspices of the Smithsonian through grants of money, the loan of material from the museum, and in other ways.

It seems to me that the Smithsonian is already the 'Nucleus of a great national * * * institute of research.' That with sufficient funds much more might be accomplished goes without saying; certainly it would be well to have a permanent corps of investigators who should not be hampered with routine administrative duties, and also tables where college

men could come for a time to carry on their investigations, though how a professor could leave his college duties for a 'term of *two* years' is not evident to me.

But why give up the museum feature of the Smithsonian? Certainly the United States should not be without a national museum. And if the museum were given up, what would be done with the great collections already there, and with the magnificent building now under construction? Used simply as a research laboratory this building would accommodate all the investigators in the entire country. With such a start as has now been made it would seem a great pity to discontinue one of the most popular and instructive attractions of the national capital, and to distribute to other museums the exhibition and working collections there brought together.

In the first article noted it is stated that 'In our universities the pedagogic element is predominant to a degree quite unknown in the German universities, and the body of investigators in them in any one field is too small to create that which is the most stimulating thing in all research—an atmosphere of investigation.' It is certainly true that most of us who are in university work are heavily burdened with pedagogic duties; but President Gilman once said, 'Sterile intellects attribute their non-productiveness to overwork, when a more acute diagnosis detects a lack of will-power.' The statement in the above quotation in regard to the absence of the 'atmosphere of investigation' in American universities seems to me to be rather sweeping. Of course in a majority of our colleges the number of men in each department is so small that it is difficult to create an atmosphere of investigation, but that there is such an atmosphere in many of our best institutions is an undoubted fact.

In conclusion, I should say—let the Smithsonian *continue* to be the nucleus of a great national institute of research, and, without diminishing the importance of the museum feature, let sufficient funds be made available to carry on the additional work suggested in the two articles quoted. ALBERT M. REESE.

SYRACUSE UNIVERSITY.

SPECIAL ARTICLES.

YELLOW MICE AND GAMETIC PURITY.

THE attention of readers of SCIENCE has been directed by Professors Morgan (1905) and Wilson (1906) to the curious method of inheritance of yellow pigmentation among mice, according to observations made by Cuénot (1905). Cuénot found that yellow in mice behaves as an ordinary Mendelian character dominant over all other types of pigmentation, but peculiar in that it can never be obtained in a homozygous condition, yellow mice forming regularly two sorts of gametes, one sort being yellow, the other sort being in some cases gray, in other cases black, and in still others chocolate.

These surprising observations carry with them important theoretical conclusions. Already they have been interpreted in ways very different by Cuénot and by Morgan. A fuller knowledge of the facts may show which interpretation is correct, or whether possibly neither is adequate without some modification. It is important first fully to establish the facts. With this idea in mind (and, I confess, inclined to be sceptical because I had found yellow so different in behavior in guinea-pigs and rabbits from what Cuénot reports it to be in mice) I have recently made a reexamination of some breeding records of fancy mice, reared in 1900–1901, in connection with an investigation of sex-determining factors in mammals. The purely incidental records of color-inheritance have not previously been published, and I should hesitate to publish them now in their fragmentary condition, did they not serve to supplement and in the main to corroborate the more extensive observations of Cuénot.

My original stock of mice, obtained from a near-by breeder, consisted of the following sorts: (1) black-white spotted mice, some homozygous, some containing chocolate as a recessive character; (2) chocolate (or chocolate-white) mice, homozygous or else containing recessive total albinism; (3) yellow mice (three in number) all of a clear reddish yellow color above, but almost white below. Young were obtained from one only of the three yel-

low mice, a male (♂ C). A second male was not used in the breeding experiments. The third yellow animal, a female, was sterile by every sort of mating tried, a considerable number, which fact is of interest in connection with Cuénot's experience, to the effect that yellow mice are feeble in reproductive capacity, though vigorous and even savage. (4) The fourth category of mice consisted of a pair of *very large* albinos and their three young. Subsequent experiments showed that these albinos transmitted as latent characters in their gametes either yellow or chocolate, but in no case black pigmentation.

Other yellow mice were obtained (1) by mating the yellow male with chocolate females, (2) by mating the albinos mentioned to black animals, and (3) by mating a chocolate female with one of the albinos.

The experiments which will be described in detail relate to crosses of the yellow mice *inter se* or with mice of other colors.

Before passing to a consideration of the crosses, I should mention that the yellow mice obtained in my experiments were of two distinct types in their *adult* pelage. In their first coat all alike were of a clear yellow color, like the three original yellow mice. This character was retained in a part only of the yellow mice produced in my experiments; the others developed in their adult (second ?) coat a certain amount of chocolate pigment found associated with the yellow pigment granules, especially toward the base of the hairs. Animals of this second sort I shall call *sooty yellow*, to distinguish them from the *clear yellow* type, in the hair of which only yellow pigment is found. Sooty yellow mice are undoubtedly included in Bateson's (1903) category of 'dingy or sooty fawn' formed 'when dark pigment is present in association with predominant yellow.' Cuénot evidently had under observation both clear yellow and sooty yellow animals. He states that the yellow race contains numerous unfixable variations, not hereditary, ranging from a clear orange yellow to a sooty or grayish yellow not very different from the color of gray mice. The statement that these variations are *not hereditary* seems to me unfortunate, as seeming to imply that characters are not inherited, unless in conformity with known laws of inheritance.

The following observations corroborate Cuénot's statement that yellow is a dominant character in mice. Two clear yellow mice (♀3.2[a] and ♂3.1[c]), own brother and sister, born of a chocolate female (♀3) mated with the original clear yellow male (♂C), produced, in three successive litters, 8 yellow and 2 chocolate young. The same female bore, by another clear yellow male, 2 yellow and 1 chocolate young. Accordingly her total young by mates of her own color were 10 yellow and 3 chocolate, a close approximation to the 3:1 Mendelian ratio. It is clear that chocolate was present as a recessive character in the yellow parents. Black was as certainly *not* present as a recessive character, since it is known to dominate over chocolate, when present in the same zygote with it.

Not all of the yellow young produced by these matings had the same clear yellow color as their parents. Three of them proved to be sooty yellow, on attaining their adult coat; four remained clear yellow, like the parents; and the remaining three died before attaining the second coat, so that they can not be more accurately classified.

The recessive nature of chocolate in relation to yellow is further indicated by the following matings between a chocolate and a yellow individual, the young being in all cases either chocolate or yellow in color, never black.

Parents.				Young.			
Clear Yellow.		Chocolate.		Clear Yellow.	Sooty Yellow.	Yellow, Type not Determined.	Chocolate.
♂C	×	♀3	=	2			4
♂3.1[c]	×	♀3	=	1		2	2
♂3.1[c]	×	♀3.1[a]	=	1		3	4
♂C	×	♀3.1[a]	=			7	3
♂C	×	♀3.2[b]	=			1	2
♂C	×	♀4	=		1	2	4
♀32	×	♂49	=			1	4
Totals,				4	1	16	23

The total number of yellows produced in these matings is 21, of chocolates 23. Un-

fortunately the type of the yellow young is left uncertain by my records in all except five cases. Four of the five were clear yellows and one a sooty yellow.

The three yellow animals employed in these matings, ♂ C, ♂ 3.1[a] and ♀ 32, evidently bore chocolate as a recessive character, but not black. For if black had been present as a recessive character, it should have made its appearance as an active character in half of the young produced, but not one of the 44 young recorded in this table was black pigmented. It is perfectly clear, however, from Cuénot's statement that black *may* be present in a yellow mouse as a recessive character. That statement is supported by the following observation:

A yellow female, ♀ 35, own sister to yellow ♀ 32 previously mentioned, was mated with the same chocolate male, ♂ 49, with which ♀ 32 had been mated. She produced three young, two of which were yellow pigmented, and one *black*. The difference in the gametic condition of these two sisters is readily explained. Their mother was an albino which transmitted to each of them the dominant yellow which they manifested; their father was a black animal which bore recessive the chocolate character. Evidently he transmitted black to ♀ 35, but chocolate to ♀ 32. Accordingly the gametic formula of ♀ 35 was yellow (black recessive), but that of ♀ 32 was yellow (chocolate recessive).

The chocolate male (♂ 49), like chocolate mice in general (see Allen, 1904), transmitted no pigment character other than chocolate. Mated with three different chocolate females he produced 11 young, all chocolate pigmented. He was mated likewise with three black females, each of which bore chocolate as a recessive character. These matings produced 4 black and 2 chocolate young. In no other mating, except with yellow ♀ 35, did he produce black pigmented young.

Two matings were made between an albino (bearing latent the yellow and chocolate characters) and homozygous black individuals as follows:

Albino Parent.		Black Parent.		Yellow Young.	Black Young.
♂ 2.1[a]	×	♀ 1	=	1	3
♂ 2.1[a]	×	♀ 1.3[c]	=	2	1
Totals,				3	4

The expectation is equal numbers of yellow and of black young, on the hypothesis that yellow dominates both black and chocolate and that black dominates chocolate.

Another albino, own sister to ♂ 2.1[a] and like him bearing latent the yellow and the chocolate characters, was mated with black individuals of the same family as those used in the foregoing matings but bearing chocolate as a recessive character. The result was as follows:

Albino Parent.		Black Parent.		Yellow Young.	Black Young.	Chocolate Young.
♀ 2.1[b]	×	♂ 1.1[b]	=	2		1
♀ 2.1[b]	×	♂ 1.2[b]	=	2	2	1
Totals,				4	2	2

The expectation in this case is that young of three sorts will be produced in the proportions, 2 yellow, 1 black, 1 chocolate, which is exactly realized in the totals obtained.

A mating was likewise made between an albino, ♂ A (father of the albinos ♂ 2.1[a] and ♀ 2.1[b] used in the two experiments just described, and like them bearing latent the characters yellow and chocolate) and a chocolate female ♀ 3.2[b], used also in matings with yellow ♂ C. This mating produced one yellow and one chocolate young, three other young dying before their coat pigments were developed.

These various observations harmonize entirely with the statements of Cuénot, showing that in mice the order of dominance of the three pigments of the hair is *yellow, black, chocolate,* each dominating those which follow it in the series, but being recessive in relation to those which precede it. This order is the more surprising because in rabbits and guinea-pigs black is dominant over yellow. In guinea-pigs black is dominant also over chocolate, but in rabbits a pure chocolate is unknown.

The occurrence of two distinct types of yellows is a matter deserving further attention. The sooty yellow type may evidently be hetero-

zygous, since it may be produced by a cross between yellow and chocolate (see mating ♂C × ♀4), but clear yellow may likewise be produced in the same way and may be likewise heterozygous, as is shown further by the result of mating clear yellows *inter se*. Clear yellows so mated may produce, as we have seen, both chocolate animals and those of the sooty yellow type. Whether sooty yellows may in turn produce clear yellows, my experiments do not show, but this seems highly probable. Whether these two types differ in gametic formula is at present uncertain. The few yellow animals from which I secured young were evidently all heterozygous, like the 81 yellow mice of generation F_2 tested by Cuénot. They were also clear yellow, all except one. That one, a sooty yellow female, bore a litter of three young by a black mate; two of the young were yellow and one black.

In the hair of clear yellow animals, I have found only yellow pigment granules; but in the hair of a sooty yellow which I examined, chocolate granules occur sparingly, with the yellow ones.

Yellow heterozygotes may probably also develop *black* pigment in their adult coat, when black is the recessive character present. For Dr. G. M. Allen, when studying color-inheritance under the writer's direction, obtained a pair of yellow mice, about four weeks old, from a breeder in Washington, D. C., and within a short time these animals began to develop small spots of *black* upon the back. Unfortunately Dr. Allen was soon after forced by the assumption of other duties to discontinue his experiments with the yellow type, and since that time I have been unable to secure material for further study of the matter.

Cuénot finds in his experiments that *all* yellow mice tested by him are heterozygous, like blue Andalusian fowls (Bateson, Saunders and Punnett). He believes, what seems entirely reasonable, that this is due to infertility of gametic unions between yellow and yellow. It will be a matter of interest to see whether further investigations confirm this interpretation, or whether an explanation can be found on the ground of gametic contamination, as suggested by Morgan, or of multiplicity of factors giving rise to yellow, as to the pigmentation of stocks and sweet-peas according to recent investigations of Bateson, Saunders and Punnett (1906).

In guinea-pigs, as I have elsewhere shown (1905), yellow is recessive in relation to black, yet exceptionally a yellow-coated animal may transmit black pigmentation in about half of its gametes. The black pigmentation so transmitted is always small in amount, being in reality the black constituent of a mosaic predominantly yellow in its composition. In the parent classed as yellow (but really mosaic in nature) the amount of black is extremely reduced, or black may be absent altogether from the hair, though present as peripheral skin pigment, as is frequently the case also in yellow animals when they breed entirely true. Now the small amount of black in individuals predominantly yellow may be considered an impurity, a contamination of the self-yellow character, with black. Such contamination can be brought about by cross-breeding and exists in various different degrees. For example, (1) yellow-pigmented guinea-pigs *always* possess black-pigmented eyes. (2) They may possess also black pigment in the skin of the extremities (soles of feet, ears) but not in the hair. *Cross-breeding with blacks will increase the amount of such black pigmentation in the extracted yellows.* Animals of these two types are *recessives* in relation to self black, or to a mosaic of black and yellow; they breed true among themselves.

Again, in rabbits, yellow is recessive in relation to black, but a yellow rabbit may bear black-tipped hairs on the ears and nose, indicating the presence of the black character in a greatly weakened condition. The black impurity, however, is not in a condition of recessiveness. Yellow rabbits never, in my experience, produce black ones. But two yellow rabbits may produce a brown one, that is a yellow rabbit with sooty 'peripheral' black pigmentation mixed with the yellow. These various facts are mentioned to show that yellow animals may contain traces at least of the

black character, even when yellow is recessive to black.

The assumption which underlies the explanation of color inheritance given by Cuénot, and adopted by Bateson, is that recessives *lack altogether* a certain factor necessary for the production of the dominant pigment; that albinos, for example, have one factor necessary for the production of pigment, but lack a second factor *altogether*. Now granting that two such factors exist (they may or they may not), it is perfectly certain that many albinos possess *both* of them. For albino guinea-pigs and Himalayan albino rabbits actually do form hair pigments. There is nothing *altogether absent* from them which is a necessary factor in pigment production. In such cases, what distinguishes an albino mammal from any other sort, so far as our present knowledge goes, is a peculiarity in the distribution of the pigments over the body. Albino mammals lack pigment in the eye; what pigment they form is found at the extremities of the body.

Again, white-plumaged birds do not *lack altogether* some factor necessary for pigment formation. They invariably have pigmented eyes, but commonly lack pigment in their feathers. Nevertheless, the purest strains of white fowls are proverbially prone to form a 'black feather' occasionally. Can we say that the character, black plumage, is *altogether lacking* in white fowls? It is not present as a recessive character—I speak of established races, not cross-breds. Shall we say, with Cuénot, that an occasional black feather is a variation not heritable? By no means. In the white fowl with a black feather the black character is present, *every factor* of it! Those factors were likewise present in the white-plumaged parents of the bird in question, but they functioned less actively, so that no feather may have shown the black, though pigment was formed in the eye. The hypothesis of *absent factors* is inadequate to explain the observed facts, in at least a majority of known cases. By cross-breeding and selection we can alter the proportions of the different pigments in the coat *without eliminating* any. By repeated crossing of black with yellow, in guinea-pigs, we can weaken the activity of the black, so that while black pigment is still formed all over the body, it is formed in *less amount*. The black pigment is found in greatest amount at the extremities. If the process of weakening the black pigmentation is capable of being carried to its logical conclusion (a matter still under investigation), black pigment should finally disappear except at the extremities. Again, cross-breeding albino guinea-pigs with blacks increases the amount of black pigmentation formed at the extremities by the albinos, and induces a slight pigmentation of the coat generally, as I have elsewhere shown. How far the contamination of the albinos can be carried, I am unable as yet to say.

Facts such as these are difficult to explain on the hypothesis of two or more factors separately heritable, unless we suppose further that those factors are inherited in varying degrees or amounts. Explanation is rendered still more difficult when we come to consider characters other than pigments, such, for example, as polydactylism. For this reason I have carefully avoided incorporating the terminology of the 'factor' hypothesis into my published papers on heredity, and have referred to characters as more or less completely *active*, or in some cases as *latent*, a usage in harmony with that of Tschermak (1906). Bateson, Saunders and Punnett (1906) have recently criticized this usage, but I believe time will justify present caution in the adoption of the factor hypothesis. Morgan (1905) has attempted to combine the latency idea with the factor idea, but with results not very satisfactory from either point of view.

It seems to me, rather, that we must recognize, along with the fundamental principle of unit characters in heredity, the fact that characters may exist *in varying states of activity*.

The presence of one character often *inhibits* the activity of another. When in guinea-pigs the characters black and yellow are present together, yellow is largely inhibited. If yellow is made more active by repeated crossing with homozygous yellow, black is partially in-

hibited. In mice the relation of these pigment characters is apparently reversed. If in mice black and yellow are present together, the activity of *black* is largely inhibited; *wholly* so in the first coat and sometimes in the second, though not in the eyes; *partially* so in the adult pelage of sooty yellows. We can not suppose that some *factor* of black, wholly absent in the young, has been supplied in the adult. If so, whence has it come? Nor can we suppose that any *factor* of black is absent in either of two yellow mice, which produce black offspring. Black was not *absent,* but *inhibited!* Removal of the inhibiting character, yellow, allows black to resume activity in the young. And yet we can not suppose that yellow is in mice invariably inhibiting in its action on black. For yellow pigment, as well as black, is formed in the coat of wild gray mice. And Cuénot recognizes that yellow may 'vary' to such an extent as to be very similar to the gray of wild mice. Black is inhibited (dominated) only by yellow in that peculiar state of activity which it has in self-yellow mice, or, as I have elsewhere (1905) expressed the matter, in that condition of *'relative potency'* which it has in yellow mice. It is not impossible that a black race of mice could be found (or created) in which black had sufficient potency to dominate ordinary yellow, as it does in guinea-pigs and rabbits. Or, what would amount to the same thing, a yellow strain might be formed so lacking in relative potency (inhibiting effect) that in crosses with it, black would remain fully active. I have pointed out (1905) one similar case (of partially reversed dominance) in which certain smooth guinea-pigs form gametes of such potency as to partially inhibit in their cross-bred young the usually dominant 'rough' character of the coat. That the action in question was a temporary one, one of *inhibition,* was shown by the fact that the rough character could be recovered in full intensity in generation F_2.

Bateson, Saunders and Punnett have also shown recently (1906) that white plumage in poultry, ordinarily a dominant character, may in certain strains function as a recessive. The different behavior of yellow, in relation to other pigment types, in mice and in guinea-pigs, becomes less surprising when we learn that within the same species the relative potency of a character may vary to such an extent that it functions sometimes as a dominant, sometimes as a recessive character, in crosses with the same alternative type.

In several recent papers I have pointed out the fact that the theoretical 'purity of gametes' of Mendelian inheritance does not exist. No more does the *purity of factors* exist. We can not avoid the idea of impurity of the gametes, by introducing the conception of 'factors,' for the factors are as certainly impure as the gametes.

To sum the matter up, it is certain that unit characters exist, but it is equally certain that the units are capable of modification; gametic segregation certainly occurs in some cases (Mendelian inheritance), it does not occur in others (blending inheritance); *factors* of characters certainly exist, when characters are demonstrably complex and result from the coexistence of two or more simpler ones, as, for example, a purple pigmentation due to coexistence of red and of blue chloroplastids in plants. But let us in no case introduce more factors into our hypotheses than can be shown actually to exist.

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W. E. CASTLE.

ZOOLOGICAL LABORATORY, HARVARD UNIVERSITY, July 23, 1906.

PRELIMINARY DESCRIPTION OF TWO NEW SPECIES OF THE GENUS DICERATHERIUM MARSH, FROM THE AGATE SPRING FOSSIL QUARRY.

AMONG the commoner forms obtained by field parties of the Carnegie Museum in this rich deposit of bones, which has previously been described,[1] are two new species, provisionally referred to the genus *Diceratherium*.

The fact that *Diceratherium* is found in this horizon, which has been regarded as late Tertiary (by Marsh as Pliocene) is highly interesting, and it was thought by the present writer (*l. c.*, p. 491) that it might possibly belong in the lower Miocene. The study of the new material reveals the fact that the animals here represented are apparently somewhat more modified than the Diceratheria from the John Day beds.

The geological horizon in which this quarry is situated is at the top of the Harrison beds (Dæmonelix beds of Barbour, or the upper Arikaree of Darton). This entire section, the Gering, the Monroe Creek, the Harrison and the Nebraska beds, which overlie the Oligocene, should, I think, be regarded as lower Miocene, though the Nebraska beds may possibly represent the middle Miocene.

Diceratherium niobrarensis, n. sp.

The type, No. 1,271, of this species is a well-preserved skull, with the nasals disarticulated at the fronto-nasal suture. The nasals were found in the talus below the point where the skull was obtained, and presumably belonged to it. The premaxillaries are wanting, as is also the posterior portion of the left zygomatic arch. Except P^1 and the incisors the dentition on both sides is beautifully preserved. The principal characters of the type are as follows: Skull mesaticephalic.

$$I\frac{1}{2?},\quad C\frac{0?}{0?},\quad P\frac{4}{3?},\quad M\frac{3}{3?}.$$

The brain-case is large, the occiput high, and there is a well-formed sagittal crest with a narrow and rather shallow groove in the median line. The frontals are quite broad and convex laterally. When the nasals, which undoubtedly belong to the same specimen, are placed in position, they, with the frontals and parietals, unite in displaying an antero-posterior saddle-shaped concavity on the top of the skull. Near the anterior extremity the nasals are provided with a pair of well developed horn-cores, about 24 mm. long, rugose and subtriangular in section. Anterior to the base of the horn-cores the nasals are rapidly abbreviated, terminating in a blunt point projecting forward and downward. The narial openings, the foramen magnum, and the orbits are large. The infraorbital foramen is situated above the anterior part of P^3. The zygomatic arch is rather slender. There is no large tubercle on the supra-occipital as in *Diceratherium armatum*, but there is a prominent median ridge, which continues from near the superior margin of the foramen magnum to the top of the inion. The foramen magnum is subtriangular in outline. The occipital condyles are large and well separated by a broad median notch inferiorly. The par-occipital and postglenoid processes are very prominent, and their bases almost touch one another. The posterior nares extend forward to the anterior part of M^2. As has been stated, the first premolar is unfortunately not present in the type, but another individual of the same species (No. 1,273) shows this tooth much more reduced than in *Diceratherium armatum*. All the teeth have internal and external cingula, and are otherwise quite similar to those found in the species from the John Day.

[1] *Annals of the Carnegie Museum*, Vol. III., pp. 487–494, 1906.

MEASUREMENTS.

Greatest length of skull, approximately	450
Length of skull from occipital condyle to and including P^2	370
Length of skull from occipital condyle to M^3	190
Greatest transverse diameter of skull	235
Greatest transverse diameter of brain case	130
Greatest transverse diameter of frontals	150
Greatest transverse diameter of occipital condyles	103
Greatest transverse diameter of palate	68
Vertical diameter of the orbit	60
Length of 2d, 3d and 4th premolars and the molar series	185
Antero-posterior diameter of P^2	26
Transverse diameter of P^2	29
Antero-posterior diameter of P^4	32
Transverse diameter of P^4	36
Antero-posterior diameter of M^1	39
Transverse diameter of M^1	37
Antero-posterior diameter of M^3	35
Transverse diameter of M^3	39

Diceratherium Cooki.[2]

This species is very abundantly represented in the Agate Spring quarry, and there are at present some forty or fifty skulls, jaws, and other portions of the skeleton representing it in the Carnegie Museum. Only a small portion of this material has been freed from the matrix. Preliminary observations have been made upon eight skulls, to one of which the lower jaws are attached. Of this series a specimen designated as No. 1,572 (Carnegie Museum Catalogue of Vertebrate Fossils) is selected as the type. The skull represents an animal much smaller than *D. niobrarensis*. The top of the skull is perfect, but the anterior part of the maxillaries is wanting. The premaxillaries are lost, and the base of the skull has received considerable injury. Only the first and second molars are present on the right side, while the dentition of the left side, except P^1, is present.

The principal characters of the type are as follows: Skull mesaticephalic.

$$I\frac{1}{2}, \quad C\frac{0}{0}, \quad P\frac{4}{3}, \quad M\frac{3}{3}.$$

Brain case relatively as large as, or even larger than in *Diceratherium niobrarensis*. The occiput is rather low. The inion is broad, with a wide posterior emargination. The temporal ridges are quite prominent, not uniting to form a sagittal crest, but continuing separate as far as the inion, where they join the lambdoidal crest. The frontals are broad, especially over the orbits, and are slightly convex from side to side. The nasals are each provided with a heavy, ovate, rugose horn-core near their free extremities. The nasals are very abruptly pointed in front of the horn-cores, and are at this point directed downward and slightly forward. The dorsal surface of the skull as a whole is saddle-shaped, as in *Diceratherium niobrarensis*, but the frontals in the present species are relatively considerably broader. The zygomatic arch is quite heavy, with a rugose enlargement at the posterior angle. The narial openings are large, the posterior orifice extending forward to a point opposite the line between M^1 and M^2. The foramen magnum is rather large in size. The orbit is relatively smaller than in *Diceratherium niobrarensis*. The infra-orbital foramen is placed above P^3.

The base of the skull presents some interesting features showing a wide difference from the first species which is described in this paper. Of these the most important is the complete enclosure of the ear by the post-glenoid process and the mastoid, which touch each other in a manner somewhat similar to that described in *Ceratorhinus* according to Cope.[3] Furthermore, the dentition in the present species is distinctly more specialized than in *Diceratherium niobrarensis*, so that together with the small size of the skull it shows a degree of resemblance to European forms as *Diceratherium minutum* Cuvier. The first premolar in the present species is reduced in about the same proportion as that in the species previously described. The crotchet of M^3 has nearly closed the interspace between the cross-crests, and in very old individuals it is in fact an enclosed cavity of the tooth.

Characters worthy of being noted are derived from a number of lower jaws belonging

[2] In recognition of the kindness of Mr. James H. Cook to the field parties sent out by the Carnegie Museum.

[3] *American Naturalist*, Vol. XIII., p. 771, 1879.

to this species. The more important are the minute pair of median incisors having a rounded enameled crown, seldom showing any wear, and the very heavy and outwardly flexed angle of the lower jaw. A lower jaw from the Protoceras beds, which Professor Osborn figured,[4] suggests this very heavy angle, and it would be interesting should we be able to trace the present species to *Aceratherium mitis*.

MEASUREMENTS.

Greatest length of skull	350
Length from occipital condyle to and including P^2	307
Length from occipital condyle to M^3	150
Greatest transverse diameter of skull	215
Greatest transverse diameter of brain case	107
Greatest transverse diameter of frontals	140
Transverse diameter of nasals back of horn cores	65
Transverse diameter of nasals at the horn cores	70
Transverse diameter of palate at M^3	55
Vertical diameter of the orbit	30
Antero-posterior diameter of premolars two, three and four	68
Antero-posterior diameter of the molar series	90
Antero-posterior diameter of P^2	22
Transverse diameter of P^2	23
Antero-posterior diameter of P^4	28
Transverse diameter of P^4	29
Antero-posterior diameter of M^1	34
Transverse diameter of M^1	32
Antero-posterior diameter of M^3	26
Transverse diameter of M^3	32

O. A. PETERSON.

CARNEGIE MUSEUM,
August 15, 1906.

BOTANICAL NOTES.

SOME RECENT BOTANICAL BOOKS.

SEVERAL months ago there came from the hand of Professor Coulter another book for the use of pupils in the secondary schools. That it presents the subject with accuracy and good judgment goes without saying, for when a master in a subject writes a text-book this fact alone is a guarantee of its high standing. The present work, which bears the name of 'A Text-book of Botany' (Appleton), is a new edition, or rather a rewritten form of the widely used 'Plant Studies,' which in turn was an abridgment and combination of 'Plant Relations' and 'Plant Studies.' In the preparation of the present book Professor Coulter has made use of the suggestions and criticisms of many experienced teachers, in order to more accurately adjust the presentation of the matter to the conditions found in the secondary schools. The plan of the work can be best told in the author's own words:

> In the first five chapters the structure, function and relationships of the most obvious plant organs are considered. The purpose has been to use the most easily observed material to give preliminary training in observation, and some conception of the activities of plants. The following thirteen chapters present an outline of the plant kingdom in the simplest possible form to be at all adequate. In these chapters the morphological point of view necessarily dominates, but not to the exclusion of the physiological and ecological. In this presentation of the great groups, which is also an outline of classification, there have been included special accounts of forms of economic interest; not only because such forms as well as any others may illustrate groups, but chiefly because there is a growing conviction that Botany in the schools must relate pupils to their common experiences, as well as train them in science. For the same general reason the brief chapters on plant-breeding and forestry have been introduced. The four closing chapters include a very brief account of plant associations, the most inclusive view of plants. * * * It can not be repeated too often that this book will not serve its purpose unless it is used as a supplement to the teacher, to the laboratory and to field-work.

This is certainly an admirable statement of the purpose of botany in secondary instruction. The illustrations are numerous (320) and good, and the text is clearly written. It should prove most useful in the public schools.

In Margaret Slosson's 'How Ferns Grow' (Holt) we have quite a different type of book, this being intended for the general reader instead of the public school pupil, and therefore lacking the pedagogical form of presentation. The book is a popular manual of selected species of the ferns of the eastern United States, illustrated by forty-five plates

[4] *Memoirs A. M. N. H.*, Vol. I., p. 139, 1898.

which include a larger number of figures, mostly life-size. The author's purpose has been to give especial attention to the young fern from the time its first leaves appear up to the mature plant. These stages are illustrated by numerous figures, and these are supplemented by popular descriptions. The book will be very helpful to the class of people for whom it is primarily designed, while at the same time it will be useful to the scientific botanist who will find here much exact information about young ferns that he can not find elsewhere.

Somewhat like the last is Laing and Blackwell's 'Plants of New Zealand' (Whitcombe), which is designed 'to give an account of our native plants that will be intelligible to all.' It makes no attempt to notice or describe all of the species, dealing only with the more conspicuous flowering plants, and omitting the grasses and sedges, in addition to 'certain less important orders.' The introduction of nearly sixty pages includes a general discussion of the vegetation, in which the author has packed a great deal of most interesting information, although such words as 'formation,' 'zonation,' 'succession,' 'alternation,' 'facies,' etc., are conspicuously absent. Nor do we find that much-abused and long-suffering word 'ecology,' although the book is full of what it is now the fashion to call by that name. In short, this is an entertaining and instructive account of the plants of New Zealand told by men who have sufficient mastery of the English language to be able to write with clearness and accuracy. The book reminds us of 'Minnesota Plant Life,' written by Professor MacMillan, also a master of English, though by no means wanting in ability to write most technically on occasion. There is the same careful selection of material, the clear treatment and wealth of beautiful and apt illustrations. May we not here express the hope that Messrs. Laing and Blackwell may long continue to study and write about the plants of their country, and that New Zealand botany may not have to suffer such a loss as that involved in the retirement of Professor MacMillan from active botanical work in this country. Science can not spare such men.

A STUDY OF THE COCONUT TREE.

In an interesting paper published in the *Philippine Journal of Science,* Dr. E. B. Copeland gives the preliminary results of some studies of the coconut tree which he has made, especially with reference to its water relations. Incidentally a good many interesting facts are brought out in the course of the discussion. Thus even on large trees the roots are only about a centimeter in diameter, and they spring in great numbers (on large trees as many as 8,000) from the convex or obconical base of the trunk, which itself may reach a depth of 50 centimeters. These slender roots may be 5 to 7 meters long, and are simple, or branched at right angles to the root axis. They are very strong and elastic, and attach themselves to the soil with such firmness that the tree is never uprooted. The stem may reach 80 centimeters in diameter, and is so elastic as rarely to be broken even in the fiercest storms, although the favorite habitat of the tree is the seashore where it receives the unbroken force of the tropical winds.

Careful studies of the leaf were made, especially with reference to its loss of water (transpiration). These show that older leaves (one year old) lose more water than young leaves which have just reached their full size. This result agrees with observations made by Bergen on the leaves of the myrtle (*Myrtus*) in Italy. Some rough estimates were made as to the total amount of water transpired by the tree annually, which show that for an average tree it is more than 10,000 liters. This is a remarkably large amount when we consider the rather limited leaf area exposed by the tree.

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DISPUTED VESUVIAN ERUPTIONS.

Nine eruptions of Vesuvius are commonly admitted to have occurred during the first twelve centuries of our era, the volcano afterwards remaining quiescent until the paroxysmal outburst of 1631. Two minor disturb-

ances, however, are reported during the interval of repose, one in 1306, the other about 1500, but the authorities for both are usually discredited.

The reason for regarding the alleged fourteenth-century eruption as apocryphal is that the sole author reporting it, F. Leandro Alberti,[1] after mentioning the date adds that it happened 'when Benedict IX. was pope, and Conrad II. emperor.' As this parenthetical remark fixes the period between 1033 and 1039, critics are no doubt right in assuming that the passage applies to the eruption of 1037. Besides, supposing the event actually to have occurred in 1306, it would be strange if Petrarch and Boccaccio failed to allude to it in their geographical works of the same century, when speaking of Somma and Vesuvius. References to these authors will be found in Enrico Cocchia's interesting essay on the ancient form of Vesuvius, reprinted in volume III. of his 'Saggi Filologici' (Naples, 1902).

Our only authority for an early sixteenth-century eruption is Ambrogio di Leone, a learned physician, professor of medicine at the University of Naples, afterwards a resident of Venice, close friend of Erasmus, and highly esteemed scholar and author. Of him says one of his biographers: "Visse sempre onorato dagli eruditi e da' Signori, e singolarmente da Leone X." No reason has been assigned for doubting his statement that ashes were ejected for a period of three days, covering the country about Nola, except that contemporary Neapolitan writers make no mention of the occurrence. Their silence is the less surprising, however, considering the relative unimportance of the event, and the fact that the fall of ashes was in the opposite direction from Naples. It is possible, also, that the discharge took place from the northern side of the mountain, away from the city and invisible from it, instead of from within the crater itself, which was observed in the following century to be heavily wooded.

Leone, in his 'History of Nola' (Venice, 1514), was not only the first author to discuss the etymology of the name Vesuvius, giving abundant classic references, but also the first to portray the mountain since Pompeian times. The view given is far from crude, being from the hand of the famous engraver Girolamo Mocetto, and it is noteworthy that the two summits are shown of about equal height. Later in the century we find two authors, Agricola and Francis Scot, reporting that the Somma cone overtopped Vesuvius, but their statements are at variance with the known condition of the summits prior to the great eruption of 1631. Probably the best estimate of their respective altitudes before and after this event is that given by Schmidt, in his 'Studien über Vulkane und Erdbeben,' pp. 215–218 (Leipzig, 1881).

Accepting Leone's testimony as trustworthy, though lacking in details, there are one or two circumstances connected with the eruption which deserve mention. In the first place, it is interesting to learn from contemporary documents which have been published for the first time within recent years that the disturbance was preceded by a severe earthquake, in which two hundred and sixty inhabitants of Nola lost their lives and much property was destroyed. Following is an account of the disaster, taken from the late Bartolommeo Capasso's publication of the original:[2]

A li 6 di xbre 1499 se sentio no forte terremoto a le 6 hore de la notte, dove cadero multe case et ce morse assai gente, cioè circa 24 jentil' uomini, et 236 populani; dove che fo veramento gran pietà lo vedere tante povere persune cossì crudelemente ammazzate et sepelite sotto le fabriche, huomini, femine et certe figliole, che stavano appise a lo pietto de le meschine matri, che fo no pianto uneversale ad mirare quello miserabile spettacolo. * * *

Furthermore, we learn from various independent sources that Vesuvius was not wholly inactive during the greater part of the sixteenth century. Gonsalo Fernandez de Oviedo y Valdes, who ascended the mountain in 1501,

[1] 'Descr. Ital.,' Venice, 1561, 2d ed., 1577.

[2] *Archiv. Storico Prov. Napoletane*, Ann. VII. (1884), p. 107. Also in his 'Fonti della Storia delle Provincie Napolitane,' p. 239 (Naples, 1902). This earthquake is unrecorded in scientific literature, not even in Baratta's exhaustive catalogue, 'I Terremoti d'Italia' (Turin, 1901).

observed smoke issuing from the crater, and was told that flames were sometimes visible at night. Fumeroles were seen within the crater by Stefano Pighio in 1582, by Braccini in 1612, by the physicians Magliocco and Nicola de Rubéo, and by Padre Salimbeni, all in 1619. It will also be recalled that the year 1538 is memorable for the Monte Nuovo upheaval, one of the recorded features of which was a violent radial blast, comparable in a small way, no doubt, to the frightfully destructive agency which played such a tragic part in the West Indian catastrophes, still fresh in memory. Such being the condition of things in the sixteenth century, we have every reason to regard Leone as a credible eyewitness of the eruption which he claims to have observed from Nola in 'nostra vero tempestate.'

Finally it should be said that we owe to the well-known editor of the 'Monumenta Neapolitani Ducatus' a valuable list of references to mediæval chronicles in which several early eruptions are mentioned. Capasso's revision of the dates makes some changes necessary in the accepted catalogues. Thus, reckoning the eruption of 685 as the fifth on record in history, the succeeding ones should be referred in order to the following years: 968 or 969, 999, 1007, 1037, 1139, *circa* 1500, 1631. There are no contemporary authorities for the eruptions sometimes quoted for the years 1024 (Capaccio), 1049 (Lyell, Phillips), and 1184 (Mallet).

C. R. Eastman.

SCIENTIFIC NOTES AND NEWS.

The University of Greifswald has, on the occasion of the celebration of its four hundred and fiftieth anniversary, conferred the honorary degree of doctor of philosophy on Professor William M. Davis, of Harvard University.

At the meeting of the British Medical Association, held last week at Toronto, the degree of doctor of laws was conferred by the University of Toronto as follows: Professor Thomas Clifford Allbutt; A. H. Freeland Barbour; Sir Thomas Barlow; Sir James Barr; Sir William H. Broadbent; H. W. Langley Browne, M.D., chairman of the council of the British Medical Association; George Cooper Franklin, retiring president of the association; Professor William Dobinson Halliburton; Sir Victor Horsley; Dr. Donald McAlister, president of the British Medical Council; William Julius Mickle; M. Louis Lapicque; Professor L. Aschoff, and Dr. W. J. Mayo, M.D., president of the American Medical Association.

Dr. H. Davy, of Exeter, will be president of the next meeting of the British Medical Association to be held in that city.

Dr. W. N. Shaw, director of the British Meteorological Office, has been elected an honorary member of the Austrian Society for Meteorology.

The Technological Institute of Munich has conferred the degree of doctor of technical sciences on Dr. W. H. Perkin, in connection with the recent jubilee of the coal tar color industry.

Mr. W. Eagle Clarke has been appointed keeper of the natural history collections of the Museum of Science and Art, Edinburgh, in succession to Dr. R. H. Traquair, F.R.S.

Professor F. Cavara, of Catania, has been appointed director of the Naples Botanical Garden.

Dr. F. W. T. Hunger, of Utrecht, has been appointed director of the experimental station at Salapig, Java.

Professor Pozzi has been presented with a gold medallion portrait by the sculptor Chaplain and a memorial volume of researches by his former students on the occasion of the twentieth anniversary of his work at the Broca Hospital.

The Moxon medal of the Royal College of Physicians of London has been awarded to Dr. Jonathan Hutchinson, F.R.S.

We learn from *Nature* that at the concluding meeting of the International Conference on Hybridization and Plant Breeding the Veitch gold memorial medals were presented to Mr. W. Bateson, F.R.S., the president of the conference, Professor Johannsen, Professor Wittmack and Professor Maurice

de Vilmorin, and silver-gilt Banksian medals to Miss E. R. Saunders, lecturer on botany at Newnham College, and Mr. R. H. Biffen, for eminent services rendered to scientific and practical horticulture. Professor de Vilmorin, as the representative of the Horticultural Society and the Botanical Society of France, invited the society to hold its next conference at Paris.

UNDER the direction of Mr. Whitman Cross, Mr. Albert Johannsen, of the United States Geological Survey, is making a reconnaissance this summer with Mr. Willis T. Lee in the northeastern section of New Mexico.

A RECONNAISSANCE survey of the Paleozoic rocks in Utah is being made this summer by Mr. F. B. Weeks, of the U. S. Geological Survey. This will complete Mr. Weeks's study of the Paleozoic rocks of the state, and will furnish the final data for a paper on the subject.

MR. SAMUEL SANFORD is engaged in an investigation of the underground waters of the coastal plain of Virginia for the United States Geological Survey. The developments in this important region have been very marked since the report of N. H. Darton in 1896 and the demand for information very great, especially in regard to the quality of the water and its availability for boiler and industrial purposes. To supply this complete chemical studies will be undertaken as a part of the work.

DR. LEON J. COLE, of Harvard University, has been appointed chief of the Division of Animal Breeding and Pathology in the Rhode Island Station. J. Frank Morgan, a graduate of St. Lawrence University; H. S. Hammond, of the Agricultural College, at Guelph, Canada, and W. F. Purrington, a graduate of the New Hampshire College of Agriculture and Mechanic Arts, have recently received appointments as assistant chemists at the same station.

DR. CLEMENS SCHLÜTTER, professor of geology and paleontology, at Bonn, has retired from active service.

PROFESSOR R. HEYMONS, professor of zoology in the School of Forestry at Munster, has been appointed curator in the Zoological Museum at Berlin.

THE Bradshaw lecture will be delivered at the Royal College of Physicians, London, on November 6, by Dr. Sharkey, who will take as his subject 'Rectal Alimentation'; the Fitz-Patrick lectures will be given by Dr. Norman Moore, on November 8 and 13, and will deal with the 'History of the Study of Clinical Medicine in the British Islands'; and the Horace Dobell lecture by Dr. F. W. Andrews, on November 15, will treat of the 'Evolution of the Streptococci.'

MR. E. A. MINCHIN, M.A., the recently-appointed professor of protozoology at London University, will deliver his inaugural lecture on 'The Scope and Problems of Protozoology' on November 15.

THE Berlin correspondent of the *Medical Record* states that the physicians of Berlin are much interested in the monument to be erected in memory of Virchow. The commission has given the first prize to the design of the sculptor Klimsch, which portrays a man fighting against a fabulous monster, while Virchow's portrait appears only in a medallion on the base. The medical societies have protested against this selection, and would prefer a monument showing a full-length figure of the great pathologist. The commission has resolved to reopen the question, but no decisive action has yet been taken.

ALBERT GRANNIS LANE, assistant superintendent of the Chicago city schools, formerly president and a trustee of the National Educational Association, died on August 22 at the age of sixty-five years.

SIR ALEXANDER MONCRIEFF, K.C.B., F.R.S., known for his improvements in the system of mounting heavy ordnance, has died at the age of seventy-seven years.

MR. RICHARD GLASCOTT SYMES, for many years geologist on the Geological Survey of Great Britain, died on July 27, at the age of sixty-six years.

PROFESSOR GEORGE REYET, for the past twenty-five years director of the observatory at Bordeaux, has died at the age of sixty-seven years.

Dr. P. Hauptfleisch, docent for botany in the Technological Institute at Stuttgart, has died at the age of forty-three years.

Dr. E. A. Kehrer, associate professor of chemistry at the Technical Institute at Stuttgart, has died at the age of fifty-seven years.

The Civil Service Commission announces an examination on September 26–27, 1906, to fill several vacancies in the position of analytical chemist, qualified in methods for the detection of food adulteration, at $1,200 per annum each, and one or more vacancies in the position of scientific assistant in plant pathology, at $1,000 per annum each, in the Department of Agriculture, and similar vacancies as they may occur in that department. Competitors in this examination may, if they desire, take the examination to be held on October 17–18, upon filing new applications in due time, this examination being held in view of the urgent need of eligibles with the qualifications indicated.

Mr. Albert Willcox, an insurance broker, of New York City, has bequeathed $100,000 to the National Association of the Audubon Societies for the protection of wild birds and animals. Half of the residue of the estate is, at the death of his brother, also to go to the Audubon Society and the other half to the Tuskegee Institute.

As we have already announced, the fifteenth International Congress of Americanists meets at Quebec from the tenth to the fifteenth of September. The preliminary program contains the titles of eighty-one papers that will be presented and arrangements have been made for suitable receptions and excursions. The place of meeting, the arrangements for the entertainment of members and the scientific program promise a congress of unusual interest.

At the annual meeting of the fellows of the Royal Botanic Society held on the sixteenth inst., the old members of the council were re-elected in spite of the opposition to the present administration. The debt of the society is now £34,000. The deficit last year was £975 as compared with £1,237 the preceding year. The chairman announced that the council now proposed certain additions to the by-laws (1) to the effect that at four meetings in the year fellows might propose resolutions; (2) to enable fellows to vote by proxy; and (3) to give the council the power of recommending such persons as they thought proper to be temporarily members, associates, or otherwise affiliated to the society, and of fixing the conditions of admission, privileges and payments in all such cases, provided that due regard was had to the position, rights and privileges of existing fellows and members.

Nature states that a party of French medical men is about to visit Germany for the purpose of inspecting the medical institutions of the country. Three days will be spent in Berlin, and other cities and towns visited will include Cologne, Frankfort, Leipzig, Munich, Bonn, Heidelberg and Marburg. A committee of entertainment has been formed under the presidency of Professor von Bergmann.

UNIVERSITY AND EDUCATIONAL NEWS.

Dr. William H. Hobbs, recently professor of mineralogy and petrology at the University of Wisconsin, has been appointed to the chair of geology at the University of Michigan, vacant by the death of Professor Israel C. Russell.

Dr. O. W. Richardson, of Trinity College, Cambridge, has been appointed professor of physics in Princeton University.

Dr. J. E. Wallace Wallin, instructor in philosophy at Princeton, has been appointed professor of psychology and pedagogy in the Pennsylvania Normal School at East Stroudsburg.

Mr. E. B. Hutchins, Jr., has been elected professor of chemistry at Carroll College, where a large science hall has just been erected.

Mr. Eugene F. McCampbell has been elected instructor in bacteriology at the Ohio State University.

Professor Thoinot has been appointed to the chair of legal medicine at Paris, vacant by the death of M. Bruardel.

Dr. Alfred Philippsohn, professor of geography at Bern, has been called to Halle.